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The appendix by Thos. Ketteringham, on the preparation and mounting of objects, is useful to beginners, though somewhat more in need of revision than the body of the work.—R. H. W.

BOTANY.

SEX IN PLANTS.—The remarks of Dr. John Stockton Hough on sex in plants (p. 19, *AMERICAN NATURALIST*, 1874) are so kind and complimentary to me, that only a desire to aid science, a desire I am sure my friend will respect, leads me to offer the following remarks.

That Dr. Hough has mistaken my views is clear, from his suggestion that I should have used the word “development” in my papers. Nothing was further from my thoughts. I have endeavored to show that sex is determined before development begins; and I have used the term vitality or vigor in order to express the determining power. In a field so wholly new, as this question was when I entered into it, I had great difficulty in finding terms to represent the facts properly; but whenever I have used the terms vigor or vitality, I have always explained that I meant by them a high or low degree of life whatever that might be. If two plants or parts of plants equally “developed,” were placed under the same circumstances as regards nutrition, and one died while the other passed through uninjured, this I call a test of vitality. In the one case there is a low vital power, in the other a higher; this I have taken as the chief factor in deciding sex, and “development” has clearly no place in the idea.

That Dr. Hough has not read my papers very closely also appears from his quotations. It was I and not Mr. Darwin, who recorded the fact that female branches sometimes appeared on male silver maples; and I also gave the account of Mr. Arnold's cross-experiments, both in the “Proceedings of the Academy of Natural Sciences” of Philadelphia, before the dates he refers to. These are minor errors to be sure, but they lead to the fear that there may be greater ones; and that greater ones do occur is clear from his quoting me as saying that, “In Norway spruces it is only in the fourth or fifth year, when vitality in the spur is nearly exhausted, that male flowers abundantly appear.” I never said anything of the kind; Norway spruces have no spurs. Again I am made to build considerably on the Cupuliferæ in my arguments

on sex. I have indeed named the oak, the beech, and the hazel, among numerous others incidentally, as plants which would bear out my views; but it is in the Coniferæ, not Cupuliferæ, that I have given in detail the facts.

Any one who will read my papers, as referred to by Dr. Hough, will I am sure not agree with him that they prove his position. His proposition is, "that female plants, like female animals, are less highly developed than males, and are the result of an inferior developmental effort on the part of the female parents." In the first place there can be no comparison between female "plants" and female animals. There is an individualized vitality in the various parts of a "plant," that there is not in an animal, and that vital power which turns food into life is operating in numberless places in the plant, to the one solitary organ in the animal; and in my view it is the varying phases of this vital power as determined by nutrition, in the various and varying parts of plants, which give direction or "development" to the subsequent sex. For instance I have shown that in *Pinus*, *Abies*, *Picea*, *Larix*, and kindred forms, the female flowers are only borne on those *most favorably situated for perfect nutrition*, and that these many female branches, *after they become half dead*, commence to bear male flowers. How can this favor Dr. Hough's proposition? How can Dr. Hough's proposition be true, if I have truly stated the facts? That they are true I appeal to any one who will take the trouble to examine the trees I have named when in blossom.

I do not think that physiology alone is competent to deal with this sexual question. Morphology must go hand in hand with it. The failures to appreciate this has led my good friend into serious error in his experiments with the corn plant. If he had perceived the common truths of morphology, he would have arrived at just the opposite conclusion to that which he has. "Abridged internodes" are by no means "in other words undeveloped." There is in many plants, and especially in the Indian corn, a tremendous development going on while the "internodes are being abridged. The ear of corn is a complete branch, arrested in its *longitudinal* development. But in its embryonic condition it has more favor than the male. Every blade that forms the "husk" was destined to be a leaf, and every leaf represents a node. Let any one strip the husk from an ear, and in this way he will find that in many cases over a *score of nodes* go to make up the corn-bearing stalk.

Now examine the male branch, with its weak structure and "development," and we find that it exhausted its whole growing force in half a dozen weak nodes, with scarcely the apology for a leaf at any of the nodes. Compare this with the numerous fat husk blades, which are the morphological analogies of the leafy bracts on the male branch, and even Dr. Hough's theory of "development" fails. Then the male panicle is only a female *which has lost the vital power to combine*. If the (usually four) two ranked lower branchlets of the male panicle had the vital power to combine* with an arrested central axis, and the other high vital powers of the female ear also act, we should have an eight rowed ear of corn, instead of a male tassel. "Some of the specimens" appeared, to Dr. Hough, "as if the cob had separated into several segments," because the male tassel had gained more than usual vital force, and came nearly reaching a perfect ear. This, however, is all very clear to those who are familiar with the morphology of the corn plant, but which they may readily be excused for mistaking who have only gone so far as to imagine that "a spike (ear) is only an undeveloped branch, sometimes having two or three internodes it is true, but generally sessile. It answers very well for descriptive botany, but leads to terrible mistakes here.

In regard to Dr. Hough's facts in relation to the sexual changes in the Indian corn, I can bear testimony to their complete accuracy; and I can see that it is only his failure to appreciate their morphological value, and the real bearing of my facts on his own observations, that he has been led to regard them as favoring a view the reverse of mine.

My position is simply this — a male flower and a female flower are essentially the same in their early embryological conditions. Morphology shows that these early identical parts may take either one form (male) or another (female); and I have shown, as I claim, that the physiological law which governs this morphological development, is a higher vital power to turn nutritive forces towards the female than the male transformation—or as I have expressed it in my original paper, "It is the highest types of vitality (not gross development) that take on the female form."—**T. MEEHAN.**

* To understand how high vital power, and the ability to combine parts, go together see my paper on Adnation in Coniferae in Chicago vol. of Proc. Amer. Assoc.

A NEW RIBES.—Among the Ribes collected in Colorado Territory during the past season by Prof. John Wolf, who was acting as botanist to Lieut. Wheeler's Expedition, I find a form which appears distinct enough to have specific rank assigned it. A description is herewith sent.

RIBES WOLFII, sp. n. (*R. sanguineum* Pursh., var. *variegatum* S. Watson, King's Report, vol. v, p. 100). Shrub, neither prickly nor spiny; two to four feet high; somewhat branching; young branches light brown, minutely glandular-pubescent, angled by two slight ridges, continuing down from the expanded base of the petiole above; branches of the previous year ashy-gray with a deciduous epidermis, which, on being shed, reveals a dark brown bark beneath.

Leaves thickish cordate-orbicular, deeply 5-cleft, lobes rather obtuse, unequally serrate (though hardly doubly serrate). Average of largest leaves two, to two and one-half inches in diameter, with sinus at base one-half an inch deep. Leaves slightly viscid; under surface pale green, with a few short glandular hairs; upper surface smoother and deeper green. Petioles from one-half an inch to one and a half inches long, slightly margined by a continuation of the principal veins of the blade; expanded at base, becoming semi-amplexicaul, and *at times* with the expansion strongly pectinately-ciliate and glandular-pubescent.

Peduncles decidedly glandular-pubescent, one to two inches long, including the raceme, loosely 4 to 10-flowered. Bracts ovate-spatulate, obtuse, yellowish-white, verging to red occasionally, one to two lines long, and one line shorter than the pedicels, which are a little longer than the flowers.

Sepals red, lanceolate, one to one and one-half lines long, *never reflexed*.

Petals red, ovate-spatulate, half as long as the sepals and as long as the stamens.

Styles two, recurved, rising conically from the summit of the ovary, red for half their length and parted to, or below, the middle.

Stigmas slightly capitate.

Fruit when young, strongly glandular-hairy, but never prickly, becoming much smoother with age. Mature fruit not pulpy, maroon or reddish purple, globose, three-eighths of an inch in diameter.

Seeds few to many, distinctly margined all around; with the inner covering longitudinally punctate as seen through the gelatinous coating.

Twin Lakes and Mosquito Pass, Colorado Territory. Among rocks, at an altitude of ten to eleven thousand feet.

It will be seen that this plant approaches both *R. glutinosum* Benth., and *R. sanguineum* Pursh. It is distinguished from the former by being fewer flowered, having shorter racemes and a rounder berry; from the latter by its shorter racemes, relatively shorter bracts and longer pedicels, and erect calyx lobes.

Its nearest affinity is (as suggested also by Mr. Watson) *R. sanguineum* Pursh., of which it may be but a variety. I think it sufficiently distinct, however, to bear the name of its zealous discoverer, Prof. John Wolf. — J. T. ROTHROCK.

PERIODIC MOTIONS OF LEAVES AND PETALS.—These phenomena, on which much has been written both in England and Germany, have been the subject of a fresh series of observations by the German botanist Batalin. He divides the different instances of motion into three groups:

(1.) Rapid automatic motions caused by a special motile organ, the pulvinus, at the base of the leaf-stalk. (2.) Diurnal motions not so rapid but also resulting from a special motile organ. (3.) Diurnal motions belonging to the whole of the leaf-stalk and partially also to the surface of the leaf, but not connected with the presence of a pulvinus. The third of these classes, to which belong the motions of petals which cause the opening and closing of flowers were the special subject of Batalin's observations. The ordinary explanation of the phenomenon has been the different degree of tension in the two sides of the leaf caused by a difference in the amount of water contained in them, which explanation has however already been shown by Pfeffer not to meet all cases. Batalin agrees with Pfeffer's conclusions and he considers the main cause of the motion to be unequal growth of the two sides caused by alternating differences in the light, temperature and turgescence. He believes that the same cause is also one of those most efficient in the other classes of periodic motions connected with special motile organs.—A. W. B.

ASCENT OF SAP IN THE BARK OF TREES.—M. Faivre has recently performed a series of experiments on the mulberry, hazel-nut and cherry-laurel, which he considers go far to prove the fact that the substances which supply the food of plants have an ascending motion in the bark. For this purpose he made perfect or imperfect annular incisions through the bark, or detached pieces of the bark to which buds were attached, or removed entire cylinders of bark from the trunk. The result of the experiments was that the buds always continued their development when the communication remained uninterrupted with the lower portion of the trunk, while, when this communication was completely destroyed, the buds invariably withered away. If the bud was separated by a perfect annular incision, it withered the more slowly the greater its distance from the incision; and in these cases the starch disappeared completely from the portions of the wood above the incision between it and the bud. When entire cylinders of bark with buds on them were removed, the buds continued to develop, and even produced branches bearing leaves.—A. W. B.

BOTRYCHIUM LUNARIA SWARTZ, IN MICHIGAN.—Last summer (August 14, 1873), I found this rare fern on one of the small rocky islands which lie off the southwest end of Isle Royale, Michigan (Lake Superior), which, from its general outline, I have named Triangle Island, it being unnamed hitherto on any of the maps.

This is an important addition to the flora of Michigan; and though I am aware that the plant had already been discovered on Lake Superior, I am assured that this is the first time of its being found within the limits of the United States.

The plants, of which I collected between thirty and forty, grew on the exposed sand-rock, among matted tufts of dwarfed *Potentilla tridentata* Ait., grass, and other plants. They are remarkably fine, well developed specimens, and quite characteristic. The island is not wooded.—HENRY GILLMAN, *Detroit, Michigan*.

ABSORPTION OF AMMONIA BY THE AËRIAL PARTS OF PLANTS.—A point of considerable practical importance to agriculturists has been recently investigated in Germany, by M. Adolf Mayer of Wiesbaden, viz., whether the aërial parts of plants have the power of absorbing ammonia or not. He carried out a series of experiments on plants growing in such a manner that access of ammo-

nia through the roots was prevented, while the leaves were subjected to the influence of this substance in either a gaseous or dissolved condition. The upshot of his experiments was that a variety of plants subjected to these conditions all had the power of absorbing carbonate of ammonia by their aërial parts both in the gaseous and the dissolved condition and of employing it in the building up of their tissues. The plants did not appear however to thrive when the access of ammonia through the roots was entirely prevented. The experiments did not indicate that Leguminosæ have any special aptitude for absorbing ammonia through their aërial organs, nor for assimilating the combined nitrogen of the atmosphere.—A. W. B.

ZOOLOGY.

PET SPIDERS.—Veritable pets they were, and why not? We hear of pet cats, pet monkeys, pet toads, and an English naturalist had his tame wasp; then why not pet spiders? But without considering why or why not I had them and enjoyed them for several months. The account which I now give of them is written from notes taken several years ago. I did not then nor do I now know the genus to which these spiders belonged, but think they may have been of the genus *Lycosa*. There were two taken at different times; the first I found under a stone, the second was brought to me pretty thoroughly benumbed with wet and cold, having been taken from a tub of water. I had already provided a domicile for my first capture in the shape of a large cigar box, covered with a pane of glass, and watched with some interest its reception of a new inmate, half expecting it would make an onslaught on the weaker one and kill it for its intrusion, but it manifested no interest whatever, until, enlivened by the warmth, the new comer began to move about, then it was evidently somewhat disturbed and kept to its own side of the box, and the stranger on coming to life enough to realize the presence of its fellow did likewise. Thus for a day or two they were exceedingly shy of each other, but in the course of a week their fear wore away and they were peaceable companions enough, but this amicable arrangement promised to end suddenly, as I thought, at one time, for while I was watching them they ran toward each other; as they met, rising on their hind legs, with the fore legs of each resting on the other's head and body, with jaws widely distended, they appeared as if